Response to J Lillin: “Debating the Devil”

This video is in response to the critiques he raised concerning the 1998 Rind meta-analysis, but to sum up this entire field of CSA research up, when non-biased sampling is utilized, and the data is quantitatively reviewed, only then can you accurately assess CSA-adjustment relations. My sources will be linked in the description below.

.Wait until you find out about qualitative reviews

Whilst not directly representative, college students are extremely relevant to the population of people who experience CSA: Not only do 50% of all USA adults attend college, the results of these samples are comparable to those of national samples in terms of prevalence, severity, and correlates—which are samples, specifically selected to be representative of a nation’s population of people who experience CSA. This criticism is extremely bizarre to me because college samples are generally well accepted even amongst current victimologist reviews, the conclusions of which you support.

The whole point of the study was to test basic claims concerning CSA: including “ALL CSA typically produces intense, long-lasting harm”. The fact such harm wasn’t observed in less serious experiences directly disputes this belief. The authors then proceeded to examine just how intense is the harm produced by CSA through the meta-analytical approach.

He continues to claim the results are biased, as CSA victims actually able to make it to college would be better psychologically adjusted than non-students, thus, falsely estimating general CSA-adjustment relations. If this was true, we would see statistically significant effect sizes in non-college student samples. We do not. They’re the exact same.

Again, we can turn the years back and the effect sizes for CSA are still non-significant, and they remain non-signifiant throughout college, and past 30.

What he means by this is when a CSA victim years after the interaction falls into a bad habit, situation, or weird attitude, and it gets entirely ascribed to CSA; this is a result of the basic assumed properties of CSA which I fundamentally reject on a lack of evidence, and evidence to the contrary basis. Once the CSA-maladjustment link was first proposed, the symptomatology ballooned exponentially into virtually every mental disorder and issue because everything was blamed on CSA; I can only assume increasing this absurdly long list is what victomologists mean when they say CSA research is ever-expanding. Who knows maybe in a few years we’ll see: AIDS, rectal cancer, increased chance of stubbing your toe. Every single problem you face in life is because you touched a winky when you were 6.

But I’m still going to relate it!

Again. The whole point of the study was to test the basic claims concerning CSA. The definition used in the study is the definition accepted by many victimologists and lay persons as typically leading to intense, long-lasting harm. You're the ones who created the definition.

Except studies mainly consisting of non-contact CSA were not used in the meta analysis of CSA-symptom associations. Even if they were included, the argument would be unsupported as non-contact CSA is shown to be identical in CSA-symptom associations to physical contact. You would know this if you actually read the study. The non-contact studies, Landis, and Risin and Koss *were* used in assessing self-reported reactions, and you'll find this funny: *Weighted* means were used for these studies as to maximize negative reports, so removing the data would actually make the results stronger, which is pretty embarrassing for you.

This criticism can be applied to all CSA literatures. But if you want to talk about to whom it applies more, you could probably guess first. With the non-clinical examinations less direct language was typically used as to minimize underreporting, for example, saying experience as opposed to abuse. Compare this with clinical samples where the presupposition of examiners is that CSA is inherently harmful, where the patient is, at the end of therapy, and after incessant interrogation, always taught they're a "victim" of "horrific abuse" and require "treatment". And, in general, even outside clinical samples, biased researchers do use language, such as abuse, victim, or molestation, during recruitment and examinations which alienate a large portion of the CSA population who don't consider their experience as such, and would have no problem divulging their experiences. Let's scurry back to National samples, where the unweighted mean of response rate is 83%

Correct for what? I honestly don't know what he's talking about. I think he believes there are extraordinary cases not being represented in the college population, which are represented in the clinical and legal population. But again, there's no evidence to suggest those samples are representative in any way of the general CSA population.

Because they don't perceive their experiences as abusive, even more so than females because males receive more positive messages regarding sex. Males do report less effects than females, but researchers arbitrarily attribute this to cognitive distortions, and thus conclude the males to be negatively affected by CSA, or they select samples with extreme instances of abuse, and generalise them. I'll quote directly from Rind's direct response to this criticism: The critics argued that boys who do not perceive harm are committing misattribution, and that this has to be accounted for. They cited Myers (1989) to argue that sexually abused boys tend to deny or normalize their abuse.

Their claim of misattribution is circular in that harm is assumed to occur, and lack of perceived harm is therefore inferred to be cognitive distortion, thereby "proving" the initial assumption. Myers' (1989) research was a clinical study involving unusual cases of unambiguous and severe abuse; their use of this citation shows an obstinacy in insisting that the typical case is validly characterized by the extreme" end quote. Swap out sexual interaction with eating apples and

you will see how absolutely absurd this criticism is from an unbiased viewpoint; the existence of people who've died eating poisonous apples does not mean apples are inherently deadly. It also doesn't mean people who eat regular apples, and come out fine are denying or normalizing the fact they've just been poisoned—to your knowledge they're perfectly fine, you've observed it. The whole point of science is to observe, then make conclusions based on those observations; not to affirm preconceived assumptions.

That's how all CSA studies operate. The overall relationship of CSA and adjustment can be assessed by measuring individual symptoms. Effect sizes across samples were homogenous across 17 out of 18 symptoms, which were also uniform. It's here where I more highly doubt you even read the study. For all the symptoms assessed, family environment was a much more reliable predictor than CSA in college samples. Outlying results here are explained by the nature of how symptoms are ascribed to CSA, explained earlier, where every symptom imaginable, including those caused by neither of the datas assessed, are arbitrarily blamed on CSA, meaning those results were not functions of any of these measures.

Given the consistency of values across symptoms which measured general adjustment: school, depression, anxiety, it only makes sense these can function as a proxy for unmeasured correlates. Considering how there just isn't enough data to accurately assess some symptoms, which is why only 18 were included in the study, your claim suffers from unfalsifiability. All you're doing is clinging onto symptoms pulled out of researchers' asses and saying "Aha, you didn't measure this one! What if it's significant? Haha, you can't say there's no causality!" From the data we've observed, they either are statistically insignificant, like the others, or not functions of any measures.

You can also read the Ulrich and Daly replications of these findings which adhered to the criticisms you've raised, and more. Hell, Heather was a bullshitter anti who claimed only some of the findings were supported, despite that not being the case—typical woman. What else - the initial critics went silent. No one cared about the American Association for the Advancement of Science's positive review of the study's methodology, which came out after the congressional condemnation, and which congress themselves asked for. There is that one review by Rind in 2012 where you can just tell how extremely pissed off he is having to respond yet again, and it's not just the all-caps title, the paper itself is one the funniest things I've read.

If you're going to respond to this video, please, first go through the back-and-forths which took place following Rind et al 1998. You can find all the scientific papers on library.lol; you'll need to download the tor browser if you're on mobile.